PROCEEDINGS

OF THE

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Ninety-Second Session.

Monday, 1*st February* 1875.

Sir WILLIAM THOMSON, President, in the Chair.

The following Communications were read:—

1. On the Complete Theory of the Stone Arch. By Edward Sang, Esq.

In the investigations usually given of the equilibrium of the stone bridge, it is assumed that the strains follow the same law as in the suspension bridge, the one being a case of stable, the other a case of instable, equilibrium; and, resulting from this hypothesis, formulæ are given whereby to determine the *extrados* suitable to a prescribed *intrados*.

The load of the chain bridge is suspended by rods, and therefore acts only in the direction of gravity; it cannot influence the horizontal strain which must be transmitted unaltered from link to link. But the weight is imposed on the stone arch in a different manner. The stone which rests on the voussoir is not jointed as the suspending rod is, and may transmit an oblique as readily as a downward strain: hence the fundamental conditions of the two structures are essentially different, and require different modes of treatment.

The mason trusts greatly to the cohesion of the cement, which easily makes up for small inaccuracies of arrangement; but the maximum stability of a stone structure is attained by so adjusting its parts that each would be in equilibrium even although the surfaces had no cohesion and no friction. For this we require that the thrusts to which each stone is subjected should be in directions normal to its several surfaces, and should balance each other.

Now each arch-stone is subjected to three pressures,—one on each of its sides in directions tangent to the curve of the arch, and a third, the pressure of the superincumbent mass upon the inner end of the stone.

To put the structure in accordance with the usual supposition, we must cause the inner ends of these stones to be dressed with horizontal surfaces, in order that the pressures exerted thereon be downwards. This being done, the usual investigations would hold good, and the intrados for a rectilinear extrados would be a modification of the catenary. But the inner ends of the arch-stones are never dressed in this way; they are rough-hewn, and made parallel to the curve of the arch, and thus the deductions from the usual hypothesis are quite inapplicable.

If we suppose the inner ends to be made parallel to the arch and to be frictionless, the load resting upon them would tend to slide down the slope, and this tendency must be counteracted by a horizontal resistance from the adjoining masonry; this, combined with the gravitation of the load, produces a resultant normal to the arch. In this way the compression of the arch stones is transmitted unchanged along the whole curve, instead of being, as in the former case, augmented in proportion to the secant of the inclination; and at the same time the horizontal thrust, instead of being conveyed unchanged to the ultimate abutment course and there resisted, is distributed through the whole depth of the mason work. On investigating that form of the intrados which, on this supposition, must suit a horizontal roadway, we obtain a differential equation which can only be integrated in somewhat complex series. This curve lies inside of the circle which osculates the arch at the vertex, while the catenarian curve, resulting from the former hypothesis, lies entirely without that circle. Between these two curves, therefore, we may have a variety of intermediates, each of which may be brought strictly into accordance with the laws of equilibrium by giving to the inner ends of the arch stones an appropriate degree of inclination.

In this way we are at liberty to assume, within reasonable limits, the forms of both intrados and extrados, and at the same time are able to satisfy punctiliously the conditions of equilibrium by properly adjusting the slope of the inner surface of the arch stone. The computation needed for this adjustment is simple and obvious.

The builder, however, would scarcely think it worth his while to cut the stones truly to the shape so found; he would often prefer the usual rough-hewn surface and the great cohesion which that roughness gives; and will probably rest contented with a test for safety, which test may be found in the very simple law, that the difference between the logarithms of the tangents of the inclinations at the two proximate points of the arch should always differ by more than the logarithms of the loads imposed between those points and the vertex of the structure.

2. On the Application of Angström's Method to the Conductivity of Wood. By C. G. Knott and A. Macfarlane. Communicated by Professor Tait.

This was an account of a series of experiments made in the Natural Philosophy Laboratory of the University, to test the applicability of Angström's method of periodic variations of temperature to the determination of low conductivity. The wood was cut into discs of a standard thickness, and these were very tightly secured together, after the interposition of copper-iron thermo-electric junctions (of very fine wire). One series of discs was cut parallel, the other perpendicular, to the fibre. The results were obtained very easily, and accorded satisfactorily with those obtained by more laborious methods.

3. Notice of Striated Rock Surfaces on North Berwick Law.
By David Stevenson, V.P.R.S.E., Civil Engineer.

The well marked "crag and tail" formation of North Berwick Law has long been appealed to by geologists as a striking example of the effects of those mysterious glacial currents, which at some time have wrought such changes on the surface of the globe. The Law presents, as is well known, a comparatively bold face, or crag to the west, against which the glacial current is supposed to have impinged, while, against its eastern face, there is a gently sloping mass of gravel, clay, and stones supposed to have been thrown up by this current under "lee" of the Law, and now forming what is called the "tail." It had often occurred to me as remarkable, that so great a mass of debris should have been left by the passing current, whatever that may have been, on the eastern extremity of the hill, while it had apparently left no impression on the north and south sides, along which it must have passed. These north and south sides, in their present condition, and to a casual observer, have the ordinary appearance of rough, angular weather-beaten rocky faces, without a trace of glacial action. However, when making an engineering examination of the country around North Berwick in September last, in search of an available water-supply for the town, I found certain very distinct traces of glacial action on the northern side of the Law, which, in connection with the "crag and tail" feature to which I have referred, must, I think, be interesting to the geologist, while they may possess additional interest from their being on a steeply inclined open hill face, and not in a ravine, or on nearly horizontal or slightly inclined strata.

They extend vertically over a space of about 30 feet, indicating the action of a moving mass of at least that depth. They can be traced horizontally over a space of about 200 feet, and they range from 160 to 190 feet above the sea-level. They present the usual two-fold glacial aspect of smoothly-ground undulating surfaces, indented by occasional deep striæ or scorings. These two kinds of marking may have been made at the same, or at different periods, but the same abrading agent could not have produced both of them. The grinding or dressing, as it has been termed, of the surface is very distinctly marked, and must have been done by the passage of some dense, but yielding body, which could be moulded to the different irregularities, both vertical and horizontal, in the surface of the hill, which must obviously have been subjected to the grinding action for a considerable period before the observed effect could have been produced. The striæ again, must have been made by the passage of sharp-pointed bodies, harder than the felspar porphyry of the Law, and carried in a mass of material of sufficient density firmly to retain the sharp, rocky protuberances embedded in it, and to press them against the hillside with enormous force, so as to groove the rock face in passing. As viewed from a little distance, the scorings appear to be nearly paralled and horizontal. But on examining such as can be reached, I found, on using the clinometer, that this is by no means the case. On one patch of rock I found two striæ within 18 inches of each other, the upper of which had a dip of 4° and the lower a dip of 20°, and both markings were dipping towards the west, being the direction from whence the movement came, as indicated by the "tail" on the eastern side of the Law. But the rise in the direction of motion indicated by these two striæ may have been caused by local pressure, due to the obstructions offered to the passage of the mass by the Law. The effect of this would be to elevate the mass; and this I think points to ice carrying imbedded stones as probably the agent which has so distinctly chronicled its passage over or round the hill, while the rise on these lines indicate that the moving mass must have been under enormous pressure; and this again is perhaps sufficient to account for the cutting of the deep grooves left in the rocky face. In short, the appearances I have noticed seem to be such as might naturally result from such glacial action as Forbes has recorded, when he says, "when the ice of the glacier abuts against the foot of Mont Chetif it is violently forced forward, as if it would make its way up the face of the hill." *

The markings I have described have, till a comparatively recent period, been covered by debris, which has fallen from the upper portion of the Law, and formed a glacis at its base. The removal of a portion of this debris, extending to about 200 feet, as a quarry for road metal, has disclosed the original surface of the rock, and revealed the features I have described. A similar mass of debris extends along the whole northern and southern faces of the hill, and if it were removed, I have no doubt similar markings would be found to extend along both sides. I believe some traces of glacial action have been found at a high level on the western face of the Law; but I carefully examined the north and south faces of the hill, and could not, in their present buried up state, find

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* Travels in the Alps, page 205.

traces of markings, except at the place I have described, from which the debris has been removed. Neither could I detect markings on the rock faces immediately above the striated surfaces, but these upper faces having been exposed to the weather, and never covered by debris, might, though at one time scored, gradually lose the markings, while those in the lower portion of the hill remained protected by the debris.

The existence of these markings seems to supply another link in connecting the "crag and tail" formation with glacial action, at least in the case of North Berwick Law. From the appearances which the removal of this debris have disclosed, we are warranted in concluding, that after the passage of the ice-sheet or glacial current, the rocky face of the Law, perhaps to its whole height, depending on the depth of the abrading agent, was similarly rough-polished and scored, that these markings on places exposed to atmospheric action have been gradually destroyed, while similar markings on the base have been preserved by the covering of debris, and may now be seen almost in their original state, if not of freshness, as least distinctness of marking.

If this view be correct, it is likely that by removing similar deposits from the base of Stirling Castle, Craigforth, and other similar rocks, interesting traces of glacial action in connection with the "tails," which exist at these places would be disclosed.

4. Laboratory Notes. By Professor Tait.

a. Photographic Records of the Sparks from a Holtz Machine.

To determine the cause of the ordinary zig-zag form of electric sparks, the author requested Mr Matheson, one of his laboratory students well skilled in photographic processes, to take instantaneous photographs of the sparks of the Holtz Machine, by means of a quartz lens, in hot and cold air alternately. Several of these photographs were exhibited, and showed much greater smoothness of the track of the spark in heated than in cold air. The zig-zag appearance seems to depend upon the presence of combustible organic particles in ordinary air, but the experiments are still in process.

b. Determination of the Surface-Tension of Liquids by the Ripples produced by a Tuning-Fork.

A slight modification of a formula given by Sir W. Thomson (*Phil. Mag.* ii. 1871), shows that the period (t) of oscillation of a particle in a deep mass of liquid agitated by simple waves or ripples is

$$t = \frac{\lambda}{\sqrt{\frac{g\lambda}{2\pi} + \frac{2\pi}{\lambda}\frac{T}{\rho}}}$$

where λ is the wave-length, T the surface-tension, and ρ the density of the liquid. By producing, with the aid of a massive tuning-fork, steady ripples in various liquids all subjected to the same conditions, and measuring micrometrically the length of these ripples, the quantity T is determined with considerable accuracy from the above formula.

c. Capillary Phenomena at the Surface of Separation of two Liquids.

The only difficulty in this investigation is the selection of two liquids, neither of which will line the interior of the capillary tube so as to disturb the behaviour of the other. This was effected in various ways, most simply by employing water and sulphuric ether; for when these liquids are shaken together and allowed to come to rest, the result is the production of a very sharply defined bounding surface between a weak solution of water in ether (above) and a weak solution of ether in water (below). The observations and measurements were made with contiguous portions of the same capillary tube,—one dipping into the upper, the other into the lower, layer.

The following Gentlemen were duly elected Fellows of the Society:—

ROBERT CLARK, Esq.
The Hon. JAMES BAIN, Lord Provost of Glasgow.
Dr T. S. CLOUSTON, F.R.C.P.E.
THOMAS FAIRLEY, Esq., Lecturer on Chemistry, Leeds.

Monday, 15th February 1875.

SIR WILLIAM THOMSON, President, in the Chair.

The following Communications were read:—

1. Obituary Notice of Dr Robert Edward Grant, late Professor of Comparative Anatomy in University College London. By Dr W. Sharpey.

Dr **Robert Edmond Grant** was the seventh son of Alexander Grant, Esquire, Writer to the Signet. He was born in his father's house in Argyle Square, Edinburgh, on the 11th of November 1793. His mother's maiden name was Jane Edmond. It appears from a memorandum in Dr Grant's handwriting, that he was sent from home to be nursed, and saw little of either of his parents during his infancy and childhood. He had eight brothers and three sisters, all of whom died before him, and as none of them left any children, Dr Grant was the last survivor of his family.

When about ten years old he was placed at the High School of Edinburgh, where he continued for five years, under the tuition, successively of Mr Christison, afterwards Professor in the University, Dr Carson, and Dr Adam, the Rector, author of the well-known work on Roman Antiquities. In 1808 his father died, and in November of that year, Dr Grant became a student in the University of Edinburgh, attending the junior classes of Latin and Greek. In the following November he entered on his curriculum of medical study, and during its course attended the several classes in the Faculty of Medicine under the professors of that day. He also studied Natural History under Professor Jameson, and attended the lectures of some of the extra-academical teachers. After completing his course of medical study, he, in 1814, took his degree of Doctor of Medicine, and published his inaugural dissertation, under the title "De Circuitu Sanguinis."

In the meantime he had obtained (in May 1814) the diploma of the College of Surgeons. In November of the same year, he was elected one of the presidents of the Medical Society of Edinburgh, a place justly regarded as an honourable object of ambition among the young aspirants in the Medical School.

Rather more than a year after taking his degree, Dr Grant went to the Continent, where he spent upwards of four years. During this time he visited various places of interest in France, Italy, and Germany, and made a pedestrian tour in Hungary; but his principal stay was in Paris, Rome, Leipsic, Dresden, Vienna, and Munich, on account, no doubt, of the varied opportunities for scientific study and general culture afforded by these foreign seats of science, art, and learning. He returned to Edinburgh in the summer of 1820, and took up his residence in his native city. At a later time he became a Fellow of the Edinburgh College of Physicians, but he seems not to have engaged in medical practice; his career had taken another direction. He had early imbibed a taste for comparative anatomy and zoology, and now devoted himself assiduously to the prosecution of these branches of science, both by continued systematic study and by original research. The study of the invertebrate animals was peculiarly attractive, and at this time Dr Grant published various interesting anatomical and physiological observations on mollusks and zoophytes; and his name will always be associated with the advances of our knowledge concerning the structure and economy of sponges, to the investigation of which Dr Grant at this time enthusiastically applied himself. The pools left by the retiring tides on the shores of the Firth of Forth afforded favourable opportunities for observation, and he would spend hours patiently watching the phenomena exhibited by these humble organisms in their native element.

Dr Grant remained at Edinburgh till 1827, and in the meantime communicated the results of his various scientific inquiries to the Edinburgh Philosophical Journal and to the Memoirs of the Wernerian Society, of which he became an active member. He was also in 1824 elected a Fellow of the Royal Society of Edinburgh.

In June 1827 Dr Grant was elected Professor of Comparative Anatomy and Zoology in the newly founded University of London, afterwards University College. He was not altogether new to the work of teaching. He had some early, though brief, experience in Edinburgh in 1824, when Dr Barclay, who for some years had delivered lectures on Comparative Anatomy during the summer session, entrusted him with the part of the course which related to the anatomy of invertebrate animals. He entered on his duties in London in 1828, and in October of that year delivered his Inaugural Lecture, which was published at the time, and went through two editions. In this office he continued up to the time of his death, during which long period of forty-six academical years he never omitted a single lecture. This was a point on which he justly prided himself. Up to the last session (1873–74) he continued to give five lectures a week, but, sensible of failing strength, he proposed to reduce the number to three in the next session, which he was not destined to see. The number of students who entered to his class fluctuated a good deal, but was never large, attendance not being compulsory in the medical curriculum prescribed by the licensing corporations. In one session (1836–37) the number was fifty-six, but usually it was between thirty and forty, and sometimes much less.

After he had thus laboured for more than twenty years, the Council of the College added to the small return he received for his services an anual stipend of one hundred pounds, which was continued during the rest of his incumbency. About the same time a number of his friends, in presenting him with a microscope, in testimony of their esteem, purchased for him a Government annuity of fifty pounds. Afterwards he succeeded to some property left by his brother Francis, an officer in the Madras army, who died in 1852, so that in his later years he found himself in easy circumstances.

His leading pupils were much attached to him, and he was sincerely esteemed and respected by all. His style of lecturing was clear and impressive, with a ready and copious flow of language. Without meaning to speak of his mode of treating his subjects, we may nevertheless remark, that on one great biological question—the origin of species—he was from the first an evolutionist, and on the promulgation of the Darwinian hypothesis of natural selection he became one of its warmest adherents.

Between 1838 and 1840, Dr Grant was frequently engaged to deliver lectures at the Literary and Scientific Institutions of various large provincial towns, where his services were in great request and high esteem. In 1833 he gave a gratuitous course of 40 lectures, on the structure and classification of animals, to the members of the Zoological Society. In 1837 he was appointed Fullerian Professor of Physiology in the Royal Institution, which he held for the usual period of three years. At a later period he was appointed by the Trustees of the British Museum to the Swiney Lectureship on Geology, the tenure of which is limited to five years. In 1841 he delivered the annual oration before the British Medical Association. In 1836 he was elected a Fellow of the Royal Society of London. He was also a Fellow of the Linnean, Zoological, and Geological Societies.

Dr Grant's vacations were spent sometimes in Scotland, but chiefly abroad, in France, Germany, Belgium, and Holland. On more than one of these occasions he was accompanied by an intelligent and favourite Hindoo pupil, Dr Chuckerbutty, who afterwards became a Professor in the Government Medical College of Calcutta. Dr Grant seems to have had a special liking for Holland, which he visited and revisited several times, partly no doubt on account of its scientific institutions and zoological museums, but largely also for the sake of acquiring the Dutch language. In like manner he afterwards spent a vacation in Copenhagen, and worked hard at Danish. Indeed, it is to be noted that he had a great taste for the study of languages, both practical and philological, and spoke the principal European tongues fluently.

Dr Grant's lectures were reported in the early numbers of the "Lancet," and he afterwards published a treatise on Comparative Anatomy, which embodied the substance of them. The work came out in parts, but was not completed. He was also author of the article, "Animal Kingdom," in Todd's Cyclopædia of Anatomy. The titles and dates of his communications to periodical works are given in the Royal Society's Catalogue of Scientific Papers; they are thirty-five in number, and extend from 1825 to 1839.

Dr Grant was a devoted lover of music, and attendance at operas and concerts was one of his chief enjoyments in his latter years.

In August 1874 Dr Grant suffered from a dysenteric attack, for which at first he would have no medical advice, and although subsequently, by appropriate treatment, the virulence of the disease was subdued, his strength was exhausted, and he died on the 23d of that month, at his house close by Euston Square. He was buried in Highgate Cemetery, attended to the grave by a few old friends and attached pupils, among whom was his friend and former companion in travel, Dr Chuckerbutty, who was then in England, and two months later was destined to follow his venerated master.

Dr Grant was never married; he knew of no surviving relative. Three of his brothers, whose deaths he had recorded, were military officers. Of these, James, a lieutenant in the German Legion, fell at the siege of Badajoz in 1811; Alexander, captain in the Madras Engineers, died in the Burmese War in 1825; and Francis, captain in the Madras army, as already mentioned, died at Edinburgh in 1852.

By his will Dr Grant bequeathed the whole of his property, including his collections and library, to University College, in the service of which he had spent the greater part of his life, and to the principles of which he was sincerely attached.

2. An Illustration of the relative Rates of Diffusion of Salts in Solution. By Professor Crum Brown.

3. On the Oscillation of a System of Bodies with rotating Portions. By Sir William Thomson.

4. Laboratory Notes. By Prof. Tait.

a. On the Application of Sir W. Thomson's Dead-Beat Arrangement to Chemical Balances.

A considerable amount of time is lost in making an accurate weighing on account of the slowness of oscillation of the balance when the loads are nearly equal; and this loss of time is nearly proportional to the delicacy or sensitiveness of the balance. Hence it becomes a matter of importance to endeavour to bring the balance speedily to rest without, if possible, impairing its sensitiveness; as thus much time and labour would be saved in weighing. Several methods of applying gaseous friction for this purpose have been tried by me of late. By far the most successful consists in suspending from the beam, either within or beyond the scale-pans, two very light closed cylinders which fit very closely (but without touching) into two fixed cylinders open at the top. Applied to a long and massive beam with considerable loads in the scale-pans, and which vibrated for some minutes when disturbed, this trial apparatus brought it to rest after, at most, *three* half vibrations. It is now evident that with a properly-constructed damper on this plan, there is practically no limit (so far as rapidity of weighing alone is concerned) to the length which may be given to a balance-beam; and, of course, no limit to the consequent sensibility of the instrument.

A very instructive hydrodynamical result was observed with this arrangement. The closed cylinder, exactly balanced inside the cylinder open at the top, is made to ascend briskly by a current of air blown even vertically downwards on the centre of its upper end.

b. Photographs of Electric Sparks taken in Cold and in Heated Air.

(*Ordered by the Council to be printed in the Transactions.*)

c. On the Electric Resistance of Iron at High Temperatures.

This note details various experiments made for me by Messrs C. M. Smith and A. Macfarlane in the Physical Laboratory of the University, and has been drawn up by these gentlemen. The only part I have taken in the work has been the suggestion of the line of investigation and the forms of apparatus employed. I mention this not alone in justice to them; but also as giving independent corroboration of results formerly arrived at by myself. [*This paper will be inserted later, when the necessary diagram is ready.*]

Monday, 1*st March* 1875.

SIR WILLIAM THOMSON, President, in the Chair.

The following Communications were read:—

1. Biographical Notice of William Euing, Esq., F.R.S.E.

By Professor William P. Dickson.

WILLIAM EUING was born on 20th May 1788, at Partick, where his father had a printfield on the banks of the Kelvin. His family originally belonged to Strathendrick, which was, along with the Lennox, the chief seat of their name. Mr Euing conceived the latter (which he traced back to Domesday Book) to be connected with Eugenius, and was somewhat particular as to its being correctly spelt with a *u*. His grandfather settled in Glasgow about 1740, and was a magistrate of the city in 1767. His father went to the West Indies in 1799, and died there; whereupon Mr Euing, who was an only child, was left to the charge of his mother, and of his uncle, Archibald Smith of Jordanhill. This relationship laid the foundation of the close friendship that subsisted between Mr Euing and his cousins, Mr James Smith of Jordanhill, F.R.S.L. & E., and Mr William Smith of Carbeth-Guthrie (Lord Provost of Glasgow in 1823), during the prolonged lives of all the three. After receiving his elementary education at two private schools, Mr Euing was sent to the Grammar School, where he had as his class-fellows the late William Lockhart of Milton-Lockhart, M.P., his own cousin Mr Robertson Reid of Gallowflat, and other subsequently well known citizens of Glasgow, all of whom he survived. He entered the University in 1800 at the age of twelve, and attended the classes of Professors Richardson, Young, Jardine, and Millar. Although an earnest student, he did not complete the regular curriculum, but early entered on business in the calendering firm of Inglis, Euing, & Co., of which he soon became a partner. In 1815, in consequence of the work being too great for his delicate health (for at this period and down to 1845 he suffered much from illness) he retired, and, after acting for some years as a commission merchant, he began in 1819 the business of an underwriter and insurance broker, in which he continued at the head of the well-known firm of William Euing & Co. till the close of his life, visiting daily almost to the last his office in the Exchange.

As a merchant, Mr Euing was held in the highest esteem by all who came into contact with him for his intelligence, his soundness of judgment, his probity, and stainless honour. He was a large shareholder in the unfortunate Western Bank, and its failure in 1857 brought into play at once his excellent habits of business and his cheerfulness of temper. He carefully and promptly arranged his own private affairs with a view to the worst, and then, as directing the proceedings of the Shareholders' Committee, applied all his energies to unravel the complicated affairs of the Bank, and to retrieve as far as possible the ruined fortunes of the shareholders —a task in which he was acknowledged to be beyond expectation successful. He was very methodical in his habits, one of which was early rising; and, long after he had reached eighty years, his elastic step might have been seen almost daily in the West End Park—a mile from his house—at an hour when but few were awake.

Mr Euing was in early life somewhat shy and reserved, having in his characteristic modesty formed a more humble estimate of his own abilities, and of his fitness to take part in society, than was entertained by those who had the privilege of knowing him in after years, of profiting by his varied information and refined taste, and of observing his deep and lively interest in literary and social questions. He early set himself to the task of self-improvement; his thirst for fresh knowledge never abated; and he found a constant pleasure in its gratification. His letters written from the Continent, during his last tour a few years ago, show, I am told, the same desire to learn everything, as do his letters written in 1816, when he made his first visit to France; and many of his books, even of those lately acquired, contain memoranda indicating their perusal and evincing a marked interest in their contents. In politics he took little part. Though earnest in his religious opinions, which were formed with conscientious independence and held with firmness, he was very tolerant in spirit; and, catholic in his sympathy with all forms of Christian work that approved themselves to his judgment by their fruits, he had but little relish for controversy. Simple and unostentatious in his personal habits, he yet found pleasure in the frequent exercise of a genial hospitality, to which his unfailing cheerfulness lent a special zest.

Mr Euing was eminently successful in business, and at full liberty—so far as family ties were concerned—to follow the bent of his own wishes and tastes in the application of his wealth. He had a singularly warm and generous heart, and was early drawn by it into those walks of practical philanthropy, with which his name is specially associated in the minds of his fellow-citizens, and in which he found growing pleasure as years went on. With rare self-denial he made it a rule—to which he systematically adhered—to set apart a large proportion of his income to purposes of benevolence. Upwards of forty years ago he began to investigate the hardships connected with imprisonment for debt, and he took a zealous and important part in procuring their mitigation. Subsequently his sympathies were warmly enlisted on behalf of Sailors' Homes, and the thriving Home in Glasgow, which was to a very large extent erected by his liberality, was the object of his constant care and unwearied bounty down to the close of his life. He bequeathed to it a legacy of L.2000; and a bust, from the chisel of Mr G. H. Ewing, has just been placed by the Directors in the hall of the institution as a fitting memorial of its patron. Almost all the public charities of Glasgow received, in addition to his regular contributions, special proofs from time to time of his liberality; and equally cordial was his interest in the Bible Society, the City Mission, and other schemes to promote the good of the community.

Not less remarkable was his interest in education, science, and art. Not to speak of his services and benefactions to the Buchanan Institution, the Mechanics' Institute, the School of Art, Stirling's Library, the Botanic Garden (to which he left L.3000), and other agencies for helping the education and elevating the tastes of the people, he manifested a specially warm and constant zeal for the prosperity of Anderson's University, of which he was long the most valued counsellor and, along with his friend Mr Young of Kelly, the most conspicuous and munificent patron. He devoted much of his time to its service, cherished a lively interest in its work and in its teachers, repeatedly made large donations to its funds, and, besides founding and endowing in it a Lectureship on Music, left to it a legacy of L.6000. He was an early subscriber of L.1000 to the new buildings of the University of Glasgow; and, besides various donations during his life, he has destined the sum of L.6000 to the endowment of fellowships bearing his name, the holders of which are to conduct tutorial classes of limited numbers, more especially during the vacation, and thereby "to confer on the University some of the benefits derived from tutorial instruction at the English Universities." Mr Euing was also a liberal patron of art, and had formed a considerable collection of pictures, thirty-six of which he presented during his life to the Corporation of

Glasgow. He has now bequeathed to them the remainder, giving powers of sale or exchange, but directing the retention of at least fifty of his pictures in their gallery. His refined taste was visible also in a collection of old silver plate and china. Mr Euing was a Fellow of the Scotch Antiquarian Society, and President of the Glasgow Archæological Society. He was long an active member, and one of the last surviving councillors, of the Maitland Club, to which he presented a volume prepared by the Rev. Mr Muir of Dysart, containing extracts from the records of that burgh. He was ever ready to countenance and encourage any apparently meritorious enterprise of antiquarian authorship. He had formed a very remarkable and highly interesting collection of autographs, which, as his will does not indicate any special destination for it, will probably fall to be dispersed.

But, of all the noble forms which the gratification of his personal tastes assumed, that on which he bestowed most attention, and which he valued most—cherishing in it a peculiar modest pride—was his library. It consisted of three distinct divisions. The first contained a very large and—so far as Scotland at least is concerned—unrivalled collection of music and of works on music, amounting to several thousand volumes. Mr Euing was an enthusiast in music, and was conversant alike with its theory and practice; indeed his love for it was so intense, that in early life he was in the habit of meeting with some friends of similar tastes as a Saint Cecilia Society at, I think, five o'clock in the morning. This musical library is bequeathed to Anderson's University in connection with the Euing Lectureship in Music, along with L.1000 towards providing a fireproof apartment for its reception, and L.200 for the compiling and printing of a catalogue. The second division consisted of a still larger and invaluable collection of editions of the Bible and its parts, chiefly of the various English versions (which are very largely represented), but including also a very considerable number of Polyglott, Greek, Latin, French, German, and Dutch Bibles, and not a few in other languages, along with numerous Psalters, and Books of Prayers and Hymns, amounting to nearly 3000 volumes. This has been left to the University Library, to be retained as a special collection. The third division was his general library, amounting to nearly 20,000 volumes, which is also bequeathed, with a few exceptions, to the University Library. This miscellaneous collection possesses many features of interest. It embraces about 150 volumes printed before A.D. 1500, special collections of works printed by the Aldine, Stephanic, and Elzevirian presses, of books printed at Edinburgh, Glasgow, Aberdeen, &c., in the 17th century, as well as of Baskerville, Barbou, Bodoni, Didot, Urie, and Foulis classics (those of the Bodoni and Foulis presses being especially numerous), the first and second folios of Shakespeare and many rare first editions of English classics; a large number of privately printed works (including a great many of the reprints issued in very limited number by his friend Mr Halliwell-Phillips) numerous books on bibliography, archæology, and the fine arts, an extensive series of English minor poems, ballads, and songs; a very curious and unique collection of Broadsides, and a few MSS., including a Hebrew Roll of Genesis of great beauty. The books have been selected by Mr Euing with much care and judgment; many of them are large paper copies, or present other specialities of bibliographic interest; and most of them are tastefully bound. The value of this gift to the University cannot be estimated at less than L.10,000. Mr Euing has judiciously empowered the Senate to sell duplicates to the extent of half his general collection; and has directed the proceeds to be applied towards the maintenance and binding of the collection, or the purchase of other books to be added to it.

Mr Euing died on the 16th May 1874, closing, in the words of a relative, "gently and without suffering a long and useful life, and not leaving a single enemy." He was a Glasgow merchant of the noblest type. Others may have equalled him in the shrewdness and worth, or in the generous heart and open hand, which happily are no uncommon characteristics of the order to which he belonged; and some, of ampler resources, may even have surpassed him in the success with which they have made their wealth minister to the gratification of some particular taste; but in the combination of the highest standing as a merchant with the zeal of a philanthropist and the refinement of a connoisseur, in the many-sided excellencies of his character and the variety of his literary and artistic taste, and in the wise destination of his resources alike during life and at death, Mr Euing may well be regarded as unique.

2. On a Faulty Construction common in Skewed Arches.

By Edward Sang, Esq.

In the course of engineering works, bridges have to be thrown obliquely over rivers or over roads, and thus the construction of oblique or skewed arches is forced upon us. The skewed stone arch has not grown in favour, partly from the greater skill required in delineating and executing the forms, partly from the fact that such skewed arches have given signs of weakness. Hence an impression has gained ground that there is something defective in the principle.

The defect, however, of those skewed arches which I have seen lies entirely in an erroneous mode of construction, which, but for the cohesion of the lime, would result in an immediate downfall. The pervading principle of all good mason work is this: that the surfaces of each stone should be dressed square to the pressures transmitted by them. Now, along the ridge or centre-line of the skew this principle is attended to; the beds of the arch-stones are placed square to the line of the roadway, that is to the line of pressure there. In consequence the line of the course begins to descend on the surface of the vault; and, in continuing the descent, the architect lays off equal distances on the curves to correspond with equal distances along the crown. Hence all the courses present equal breadths measured along the lines of pressure.

The inevitable consequence of this arrangement is, that the bed of the stone becomes more and more oblique to the pressure as we come down on the haunch of the arch; the mechanical effect being just the same as if a mason, in building a wall, were to place the stones off the level. The ends of the stones, as seen on the plane of the parapet, present, in this case, equal graduations, and whenever we see the ends of the arch-stones equally placed, we may be sure that this fault pervades the whole structure. The fault is clearly seen on one side of the model of a skewed centering exhibited to the Society.

Beginning at the crown of the arch, and descending in this way, the course becomes inclined to the line of pressure; it is necessary to bend it gradually upwards from the course just described, and the problem becomes this,—"To draw upon the surface of the vault a curve which shall cross all the lines of pressure squarely." This belongs to a well-known class of problems in what is called the calculus of variations.

The nature of this curve must depend on the character of the arch; yet it has certain general features independent of that character. The chief of these may be explained in this way. If we take two closely contiguous curves, inclosing between them, as it were, a course of arch-stones, the breadth of that course, at any place, is proportional to the cosine of the inclination of the line of pressure. Hence, in every skewed arch the breadths of the stones as seen on the parapet plane, must diminish from the crown downwards, becoming at 60° from the crown just half as broad as at the top.

In the case of the circular arch, the projection of the curve upon the plane of the parapet is the well-known tractory, which is asymptotical to the horizontal line passing through the centre. Hence we cannot have a semicylindric skewed arch, because the curve of the course-joint cannot reach to the vertical part of the surface.

The nature of the true arrangement is shown on the other side of the model.

A glance at the ends of the arch-stones of any skewed bridge is enough to apprise us of whether or not the structure have been properly arranged.

3. On the mode of Growth and Increase amongst the Corals of the Palæozoic Period. By H. Alleyne Nicholson, M.D., D.Sc., Professor of Biology in the Durham University College of Physical Science.

In the first portion of this communication, the author discussed the general phenomena exhibited by the Palæozoic corals as regards their mode of growth and increase. Five chief modes of growth were distinguished:—

a. Simple calicular gemmation, in which the coralium sends up from its calicine disc a single bud, which usually repeats the process, until there is produced a succession of corallites vertically superimposed upon one another. The peculiarity of this process consists in the fact that the same calice never produces more than one bud.

b. Compound calicular gemmation, in which the primitive corallite throws up two or more buds from its oral disc, these in turn usually repeating the process, till the corallum comes to form an inverted pyramidal mass, composed of numerous corallites diverging from the base.

c. Basal or marginal gemmation, in which new corallites are produced at the circumference of the colony or along certain definite lines proceeding from the base.

d. Parietal or lateral gemmation, in which the increase of the corallum is by the production of buds at some point in the walls of the parent corallite between the lip of the calice and the base.

e. Fission, in which the growth of the corallum is effected by the cleavage of the calice of the original corallite or corallites.

Numerous examples were adduced of the occurrence of the above modes of growth, singly or in combination, amongst the Palæozoic corals, and various modifications of these processes were discussed.

The author next discussed the value of the mode of growth of the corallum as applied to the classification of the Palæozic corals, and arrived at the conclusion that much stress could not be laid upon this point unless accompanied by other distinctive characters as well. The chief grounds upon which this conclusion was based were, that allied forms in the same genus, and sometimes even different individuals in the same species, show entirely different modes of growth; that forms belonging to the most remotely allied groups often increase in the same way; and finally, that the difficulty in determining the precise mode of growth amongst some of the fossil corals is so great as often to render this test practically inapplicable.

In conclusion, the author discussed the relations between the growth of the different parts which may comprise a compound corallum, as regulating its final form and structure.

4. The President exhibited Diagrams in illustration of the Capillary Surfaces of Revolution.

The following Gentlemen were duly elected Fellows of Society:—

CHARLES WILSON VINCENT, Esq., London.
RALPH RICHARDSON, Esq.
JOHN RAMSAY L'AMY, Esq.
E. W. PREVOST, Ph.D.
JAMES SYME, Esq.
Sir JOHN HAWKSHAW, F.R.S.

The following Gentlemen were duly elected Foreign Honorary Fellows:—

HEINRICH WILHELM DOVÉ, Berlin.
AUGUST KEKULE, Bonn.
HERMAN KOLBE, Leipzig.
ERNST EDUARD KUMMER, Berlin.
JOSEPH LIOUVILLE, Paris.
JOHN LOTHROP MOTLEY, U.S.

Monday, 15th March 1875.

DAVID MILNE HOME, Esq., LL.D., Vice-President, in the Chair.

The Council having awarded the Makdougall Brisbane Prize for the Biennial Period, 1872–74, to Professor LISTER, for his paper "On the Germ Theory of Putrefaction and other Fermentative Changes," Dr Crum Brown, in requesting the Chairman to present the medal, addressed the Chairman as follows:—

Mr CHAIRMAN,—I have been requested by the Council, and I feel it a very great honour that I have been so requested, on the occasion of the presentation of the Makdougall Brisbane prize to Professor Lister, to state shortly the grounds upon which the Council have made the award.

Every Fellow of the Society who had the privilege of hearing Professor Lister's paper read, must have a vivid recollection of the interesting and admirably clear manner in which he explained an intricate series of experiments; of his hereditary ingenuity in devising and skill in carrying out delicate mechanical contrivances, and of the eloquent as well as cogent logic with which he enforced his conclusions. I wish it had fallen to one more fit to do justice to the subject, to lay before the Society an abstract or summary of this very remarkable paper.

Professor Lister's work may be considered from several different points of view.

I. As a contribution to microscopic botany, and as such it takes a very high place. A great obstacle in the way of the study of microscopic plants is the difficulty of the determination of species. Each species is liable to great variation in form, and there is a great general resemblance between forms assumed by different species. To get over this difficulty, the method of "cultivation" has been made use of—the doubtful specimen is kept and grown to see what it will become. Professor Lister in this paper describes his novel method of cultivation, in which the fungi are made to grow in various kinds of soil. Thus, two fungi growing in Pasteur's solution may resemble one another very closely; but if transplanted into milk, and allowed to grow there, a very marked difference may be produced. Or two fungi may present in one solution forms indistinguishable from one another, but one may grow luxuriantly and the other not at all, when transferred to a different solution. Such cultivation experiments are apt to fail from a character which they have in common with cultivation experiments on a larger scale. The miniature garden, like other gardens, is liable to be infected with weeds, and it sometimes happens that such a weed, or unwelcome intruder, is mistaken for the produce of the seed sown or the plant planted. These weeds grow either from seeds contained in the soil, or introduced from without, and it is essential to a successful experiment that the first be killed or removed, and the second excluded. Professor Lister secures the necessary condition of purity of the soil, perfect freedom of his solutions from all trace of life except those fungi or germs purposely introduced, and perfect security against accidental or unintentional entrance of any living thing, without interfering with the readiness of access to each experiment during its progress. This is accomplished by means of devices, of which it is difficult to say whether the complete success or the wonderful simplicity is more striking. The results obtained need not disappoint the most sanguine investigator. Professor Lister has obtained proof that Bacteria are, at all events in some cases, directly derived from fungi, of which they are merely a special development. He has been able to determine, within not very wide limits, the number of individual germs contained in a drop of water, and to show that this number is greater in warm than in cold weather, and has proved that the number of distinct species of microscopic fungi is great beyond all previous imagination. There is one special result to which I cannot omit a reference. It is, that there are certain fungi which, although rare, and, we may therefore conclude, not, under ordinary conditions, hardy, still flourish luxuriantly and increase rapidly under certain special conditions. Thus the fungus which causes the lactic fermentation of sugar, is scarcely to be found anywhere but in dairies. Boiled milk or perfectly pure milk, may be exposed to air anywhere else without undergoing the lactic fermentation; other fungi, producing different effects, will grow in it; but if milk be exposed in a dairy, this particular fungus overcomes all others, and the lactic fermentation alone takes place. Milk is the soil specially suited for its growth, but it does not appear there of itself—it must be introduced from without.

II. Another matter of great interest connected with Mr Lister's work, is the means which it will no doubt put into our hands of preparing many chemical substances. Although he has not fully investigated the various chemical changes which accompany the growth of microscopic fungi, he has shown that each species produces its own effect; and as he has taught us how to obtain specimens of each species without mixture of any other, he has put it in our power to produce specific fermentations, and study them undisturbed by the presence of other kinds of fermentation.

III. But more general interest attaches to Mr Lister's paper as a very important step in the settlement of the question: Does life ever arise from lifeless matter, or is the origin of life not as great a mystery and as far removed from the grasp of our scientific methods as the origin of matter itself? If living things never develope out of dead nature in the ordinary processes of nature, we are forced to the conclusion that they either have existed always, or have been miraculously created. It has been supposed that there is a logical difficulty in the way of proving that life does not grow out of dead matter—that to attempt to prove this is to attempt to prove a negative. But every man of science believes that the quantity of matter is constant, and that the quantity of energy is constant, although these propositions equally involve the negatives, that matter and energy never appear or disappear, but merely undergo transformations. But although there is no absurdity, there is a great difficulty in the way of proving that living beings are always produced from pre-existing living beings. It is difficult to make our experiments under precisely the conditions under which nature works, and at the same time to exclude the possibility of the presence of living beings. If we boil our liquid in order to kill its living contents, it may be said that we change its chemical character, and deprive it of the power of producing life; if we shut it up in a hermetically closed vessel, we prevent that access of air which *may* be essential to the production of life from lifeless matter. Mr Lister has shown us how we may obtain milk, urine, and blood quite free from living beings, and keep them liquid for any length of time freely exposed to air without any risk of the entrance of living things, and he has shown us that if this be done no living things ever appear in the liquid. In his experiments we see two samples of the same substance treated, with one exception, in exactly the same way; in the one sample life is abundantly developed, in the other not at all. Can any reasonable man doubt that this striking difference of result is due to the one only difference of treatment; and this difference of treatment is merely that in the one case living things have had access to the substances, in the other they have been excluded? In all other respects the two samples have been exposed to precisely the same influences. With all respect for those experimenters who, not having taken Mr Lister's precautions, have arrived at different results, I express my conviction that it has been definitely proved that life is continuous, that living matter cannot be produced by a chemical process, and that every living thing is descended from some previously existing living parent.

IV. Another aspect of this paper is of less general scientific interest, but of much greater practical importance. Mr Lister's investigation forms the scientific basis of the system of antiseptic surgery, with which his name will always be associated. The microscopic fungi, in the consideration of which we have been engaged, perform a very important function in nature. They form a brigade in nature's army of scavengers. They transform the dead matter which once formed part of organised living beings into raw materials out of which new organisms construct their bodies; they break down the complex substances, when the complexity has become useless, into simpler compounds which can be used again. They demolish the old ruins, and render their stones fit to be employed as building materials. But they not only attack the dead, they kill the weak and the dying; and while this action may be considered useful on the whole, as leaving room for the development of the strong, it is precisely the duty of the medical man to combat this tendency of nature, to support the weak that it may have an opportunity to become strong, to ward off nature's blows that the dying may recover. This is not the place to speak of the extraordinary results obtained by Mr Lister's mode of treatment, of the certainty of cure in cases which ten years ago would have been considered absolutely hopeless; my object is rather, assuming these results, to show how intimately they are connected with the scientific truths which form the basis of this mode of treatment.

It has been suggested, and I confess that I at one time thought the suggestion a good one, that instead of trying to convince surgeons of the truth of the scientific basis, Mr Lister should draw up a code of practical rules which a surgeon might follow without thinking of germs or bacteria or fungi. A little consideration will show the absurdity of this idea. A surgeon impressed with the truth of the scientific basis needs no code of rules—he sees at once what he must do, and what he must avoid. A code of rules drawn up for one ignorant of the scientific basis would be intolerably complicated, and certain to be violated. In this, as in other and higher and more general motives, faith is essential to practice; if we know the why, we can, as a rule, find out the how; and antiseptic surgery will be successful then, and then only, when the reasons for its methods are understood and believed in.

I have endeavoured, Sir, to lay before the Society some of the reasons which have led the Council to award the Makdougall Brisbane prize to Mr Lister, and I hope I have in some measure succeeded. I cannot express the satisfaction we all feel in having a paper so eminently worthy of the award.

The following Communications were read:—

1. On the Diurnal Oscillations of the Barometer.
By Alexander Buchan, M.A.

2. The Phenomena of Single and Double Vision, as shown in the Stereoscope.
By R. S. Wyld, Esq.

When we direct the axes of the two eyes to any definite object, its different parts affect *corresponding* parts of each retina, and the object appears *single*. When we squint, or do not look direct at the object, its images affect *non-corresponding* parts of the two retinæ, and the object appears double. The more widely the axes of the eyes are deflected from the object, the further asunder the double objects seem; and the less the axes are deflected the less distant from each other the double objects appear. Thus, when we hold the finger in front of the eyes while we look at a distant candle or gas jet, the flame appears single and the finger double. When we turn the eyes to the finger it appears single and the flame appears double.

The paper read to the Society in February 1871 was an attempt to prove that all the phenomena connected with single and double vision were explanable on the supposition that the nerve fibres of each retina decussate in a common cerebral sensorium, as for instance in the *corpus quadrigeminum*, which the optic nerves are known to enter; and that as, owing to the fineness of the texture, anatomists had hitherto been unable to determine the ultimate arrangement of the fibres in the brain, we were justified in making this suggestion.

Such a crossing of nerve fibres has in it nothing improbable, for there are many instances of such crossings, as for instance in the great and in the lesser commissures of the brain. There is also a similar crossing in the *medulla oblongata* of the motor nerves from the brain, before they descend the spinal cord; and there is a similar crossing of the sensory nerves where they enter the spinal cord. The supposition then of a decussation of the fibres of the optic nerve within the brain is in analogy with what we know to be of frequent occurrence in the body.

The facts which Mr Wyld now brings before the Society are these—

1*st*, When we enter two slips of white card-board, one at each side of the stereoscope, they affect *non-corresponding* parts of the retinæ, as shown in the diagram exhibited, and they appear as *two objects.*

2*d*, When we push the slips forward till they appear to overlap, the overlapping ends appear as one object, because they affect the corresponding central points of the two retinæ. If we make a mark similar in form and size on each slip, but do not approach the slips sufficiently near for the marks at once to coalesce, such marks are nevertheless frequently observed to glide closer and coalesce. This is owing to the natural tendency we have to direct the axes of the eyes to the objects examined. This causes the marks to affect corresponding points of the retinæ, and the marks consequently coalesce visually and appear as one object.

3*d*, When the slips seem to overlap, the overlapping portion appears so greatly increased in brightness that the other parts have a tendency either to disappear altogether, or they appear and disappear at brief intervals, so long as we continue to look at the central bright portion. These dim or invisable outlying parts may, however, at any time be made to reappear by simply moving the card-board once or twice up and down, and thereby exciting the attention and the retina. They may also be made to appear by winking, by moving the eyes from side to side, or doing anything to stimulate the retinæ.

4*th*, With regard, again, to the overlapping parts, it is to be observed, that though they appear at first sight to form, as it were, one single object, yet it is easy to see that this bright part is in reality a double picture containing the impression received from each eye; and so far as these impressions are not incompatible, but may be blended the one with the other, they go to form as it were a composite picture, as we know is the case with the figures on the usual stereoscopic slides, and as we may prove to be the case by making any distinctive marks on the slips of card-board, when these marks will appear distinctly visible, as if integral parts of the overlapping portion, though seen by the different eyes.

5*th*, Another important circumstance is this. When the slips are of different colours, as for instance one slip red and the other blue, or one blue and the other yellow, these colours, when caused to overlap in the stereoscope, do not produce the intermediate colours of purple or green; on the contrary, as was stated in the paper alluded to, at one time the coloured slips appear alternately visible, at another time one half of each may be visible, and occasionally, spots, or it may be only minute specks, smaller even than the fifth part the diameter of a small pin head of the one colour, will be seen shining on the ground colour of the other card-board. These particular changes seem to depend greatly on the excited or the fatigued condition of the retinæ at the time; for if we direct our attention to any conventional mark made on either of the slips presented, the excitement of the retina of the eye, caused by the act of observing the mark, immediately causes the slip on which the mark is made to become visible, and the mark appears surrounded with a patch of the colour of the slip on which it is placed.

Two circumstances then may be mentioned as certain: that in no instance do the two colours blend into an intermediate colour; and second, that we never observe the same portion of the bright overlapping portion to have at one and the same moment two different colours; parts or spots or minute specks may, as we have said, appear of the one colour, and other parts may appear of the other colour, but though the one coloured slip visually overlaps the other differently coloured slip, we never see any part at once to possess two different colours.

The conclusions to which these phenomena lead are certainly these—that there is a physical union in a cerebral lobe of the nerve impressions coming from the two eyes, and in no other way can we account for the two retinal images giving the mind the impression of but one object both in natural and stereoscopic vision when corresponding retinal fibres are excited, and of double objects when non-corresponding fibres are excited—and no other supposition will account for the increased brightness obtained by the use of two eyes than that suggested, namely, that the nerve impressions from both eyes are physically united in the sensorium.

Another conclusion to which we are led is, that though the corresponding retinal fibres are brought into juxtaposition in the sensory, yet they are not there joined or amalgamated the one with the other, seeing they do not produce the effect of an intermediate colour, but each fibre transmits to the sensory the distinctive colour and impression which it receives in the retina.

The reason why we never see any one part of the overlapping stereoscopic objects simultaneously exhibiting either two different colours, or an intermediate colour, is a matter more difficult to explain; perhaps the following may be considered sufficient explanation. If the *smallest visible point* is a point due to the impression produced by a *single nerve fibre* from one of the eyes, then, as on the supposition of a decussation of the fibres in the sensory alternate, exceedingly small specks of different colours may at any time appear intermixed, from the circumstance of the supposed *alternate juxta position* of the individual fibres from each retina in the sensory, so, if this supposition is correct, it is evidently impossible that we can ever have the impression of two different colours superimposed on the same point and at the same moment of time.

3. On the Products of the Oxidation of Dimethyl-Thetine, and its Derivatives. By Prof. Crum Brown and Dr E. A. Letts.

The difficulty experienced in determining the sulphur contained in the compounds of *dimethyl-thetine* (described in a former communication) by oxidation to sulphuric acid, induced the authors to study the effects of various oxidising agents on these compounds.

By acting on *nitrate of dimethyl-thetine* with *dilute* nitric acid, two bodies are produced. The one has acid properties, and forms a well-marked soluble salt with baryta. The other has neither acid nor basic properties. It crystallises in very beautiful needles from a hot solution in alcohol.

By acting on the base *dimethyl-thetine* with permanganate of potash solution, the same crystalline substance is produced, but the presence of the acid body could not be ascertained. The oxidation of *dimethyl-thetine* by permanganate of potash takes place in acid or alkaline solution and in the cold.

Chromic acid has no action whatever on *dimethyl-thetine* further than combining with it to form *chromate of dimethyl-thetine*—a yellow gummy substance which refuses to crystallise. The same body may be produced by acting on a solution of hydrobromate of dimethyl-thetine with chromate of silver.

Fuming nitric acid dissolves solid *hydrobromate of dimethyl-thetine* without rise of temperature, but with separation of bromine.

On warming the solution, brisk action ensues. When this has terminated, and the nitric acid has been evaporated off on a water bath, a strongly acid syrup remains, which fumes like hot sulphuric acid. This syrup also forms a soluble barium salt.

The investigation of the compounds produced by the oxidation of dimethyl-thetine and its derivatives is proceeding, and the authors trust in a short time to be able to communicate the result of their experiments to the Society.

Monday, 5th April 1875.

PROFESSOR KELLAND, Vice-President, in the Chair.

The Council having awarded the Neill Prize for the Triennial Period, 1871–74, to Mr CHARLES WILLIAM PEACH, for his contributions to Scottish Zoology and Geology, and for his recent contributions to Fossil Botany, Professor Geikie, on the presentation of the medal, addressed the President as follows:—

SIR,—The Council of the Royal Society of Edinburgh has awarded the Neill Prize for the triennial period, 1871–74, to Mr Charles William Peach, and on the part of the Council I am requested to describe briefly to the Society on the present occasion the nature of his scientific work, which has been judged well deserving of one of the Society's medals. By the terms of the original bequest this prize is reserved for the work of a Scottish naturalist. Born in Northamptonshire, Mr Peach might seem to be excluded from the list of those to whom the prize can properly be awarded. But for more than a quarter of a century he has lived continuously in Scotland, and during that time has done at least as much as any living Scotsman to extend our knowledge of the natural history of his adopted country. From the Kyles of Sutherland to the holms of Roxburgh, he has never resided in or near any Scottish county without adding something to what was previously known about its flora and fauna, either living or fossil. The Neill bequest likewise provides that the paper or work for which the prize is given shall bear date within five years previous to the award. During the last five years Mr Peach has contributed some valuable materials towards the extension of our knowledge of the fossil plants and fishes of the Carboniferous rocks of the basin of the Forth. But the Council has considered that it will best conform to the liberal spirit of the founder, Dr Neill, himself, by having regard not only to Mr Peach's work during the last five years, but to all his labours in Scotland, which have so frequently aided the researches of his brother naturalists, from whom, in his old age, every token of grateful appreciation and kindly feeling is justly due.

The naturalist, not less than the poet, is born, not made. The quickness of eye which, without effort, lets nothing escape notice; the fine instinct which divines the meaning of half-hidden phenomena, and leads on to where further successful observations should be made; the patience with which repeated failure is borne; the enthusiasm which, amid foul weather or fair, brings the observer back joyously from the cares of this world to his self-chosen task, whether it be among the treasures of land or of sea,—these are qualities which no education can supply to us, and which no want of education can wholly repress. Mr Peach has been happy in the possession of them to no common degree. Appointed more than half a century ago to the Coast Guard Service, and necessarily restricted in his pursuits by the arduous duties of that calling, he has everywhere during that extended period shown the genuine characteristics of the born naturalist. His enforced residence near the sea has been turned by him to excellent account, for he has materially increased our acquaintance with the marine fauna which surrounds our islands.

Somewhere about twenty species, and several genera of sponges, were first made known by him as denizens of British seas. He has considerably augmented our list of native hydrozoa and polyzoa. The naked-eyed Medusæ owe not a little to his attention, and one genus of them (*Staurophora*) was first introduced by him to the naturalists of this country. The Echinoderms, too, are under similar obligations to him, for besides bringing several new species to light, he found the huge *Echinus melo* of the Mediterranean on the coast of Cornwall, and supplied the twenty-armed *Holothuria nigra* to fill up the blank pointed out by Edward Forbes among the British Holothuriæ.

Since his removal to Scotland in 1849, Mr Peach has done further and most valuable work among the mollusca and fishes, adding to our fauna several species of shell as well as some fishes—Yarrell's

Blenny, Ray's Bream, and the Anchovy, for example—which were not before known to occur so far north as the seas which wash the northern shores of Scotland. In none of his labours does the true spirit of the naturalist appear more pre-eminently than in those by which he made known the nest-building habits of certain sea-shells and fishes. At Wick he noticed that the jelly-like masses of the ascidian *Leptoclinum* very frequently contained small yellow patches in the centre. Watching these, he found that the central yellow parts were really extraneous bodies, and consisted of nests containing ova. Further observation connected these ova with the slug-like gasteropod *Lamellaria*, and showed him that this shell comes every spring regularly to shore from deeper water outside, and remains two or three months for the purpose of nidification. Again, at Peterhead he made himself intimately acquainted with the family arrangements of that rather fierce-looking little fish, the 15-spined stickle-back (*Gasterosteus spinaceus*). In a rocky pool he found a colony of them, and learnt how they built their nests and deposited their ova. He watched the hatching and growth of the young until the whole colony, young and old, took to the sea. As he used to visit them five or six times a day, the parents grew so familiar that they would swim round and touch his hand, though on the appearance of a stranger they would angrily dash at any stick or incautious finger that was brought near them. The same habit of close and cultivated observation was shown by his study of the maternal instincts of the female lobster in its native haunts.

Previous to Mr Peach's transference to Wick, very little was known about the fossil plants of the Old Red Sandstone of Caithness. Many specimens had been found, but they were commonly spoken of as indistinctly preserved, and as probably of marine origin. Setting to work among the dark flagstones of that district, he eventually succeeded in forming an admirable collection, and in showing the truly terrestrial nature of that ancient flora. Within the last few years he has continued his services to fossil botany by bringing to light some new and most interesting vegetable forms from the Carboniferons strata of the basin of the Forth. He has shown, for example, the connection between the flower-like *Antholites* and the usually detached fruit, *Cardrocarpon*, and has obtained in one fossil a conjunction of microspores and macrospores.

To palæontology he has contributed several new species of fishes from the Old Red Sandstone and the Carboniferous rocks. In great measure to his perseverance do we owe our present list of the ichthyolites of Caithness and the Orkney Islands. But perhaps the most important item of his labours, in this department, at least, if we regard questions alike in theoretical geology and in the geological structure of Britain, was the discovery of fossils in the limestones of Sutherland. Previous to his observations the rocks of the Scottish Highlands were usually grouped with the so-called "Azoic" rocks, as if they belonged to a time anterior to any of the fossil-bearing formations of the country. Obscure organic remains had been indeed detected many years before by Macculloch in the quartz-rocks of Sutherland, and these were afterwards brought again into notice by Hay Cunningham. But they had gradually passed out of mind, their organic nature being stoutly denied even by such geologists as Sedgwick and Murchison. Mr Peach, however, brought to light a good series of recognisable shells and corals, which demonstrated the limestones containing them to lie on the same geological horizon as some part of the great Lower Silurian formations of other regions. It was this discovery which enabled Sir Roderick Murchison to clear up the geological structure of the Highlands, and entitled him to be the first Brisbane medallist of this Society.

In every department of natural science to which Mr Peach has given his attention he has distinguished himself as a keen-eyed and enthusiastic collector, with an almost unrivalled shrewdness in detecting what was new, and at the same time with a disinterested readiness to hand over his materials to those who had more specially studied the department of natural history to which these materials belonged. For his varied contributions to science, carried on for so long a time, with a purity of motive and a generous helpfulness towards others which have won for him the esteem of all naturalists, and with an enthusiasm which the lapse of more than threescore years and ten has left undimmed, the Council has adjudged to him the Neill prize. I beg on their part to present him to you, with the cordial wish that he may yet live for many years among us as an honoured type of the true collector and naturalist.

The following Communications were read:—

1. On the Physiological Action of Light. Part II. By James Dewar, Esq., and Dr John G. M'Kendrick.

2. On the Structure and Systematic Position of *Tristichopterus alatus,* Egerton. By R. H. Traquair, M.D., F.G.S.

The cranial osteology and the dentition of *Tristichopterus* have been hitherto entirely unknown, and we were but imperfectly acquainted with the structure of the pectoral fins. Consequently great doubts have prevailed with regard to its affinities, though it was supposed to be allied in many respects to *Dipterus*. A suite of specimens from John O'Groat's, in the Edinburgh Museum of Science, collected by Mr Peach, the original discoverer of the fish, subsequent to the publication of Sir Philip Egerton's description, throws great light on the previously unknown points of its structure, as well as on its affinities. In the osteology of the head it presents a striking resemblance to the *Saurodipterini*, and to the genus *Gyroptychius*, as described by Pander. The teeth are acutely conical, and of two sizes, large and small; the larger teeth have their bases fluted externally, and internally the dentine is seen to be thrown into a series of simple folds, the pulp cavity becoming simple towards the apex of the tooth. The shoulder girdle is provided with interclaviculars; the pectoral fin is subacutely lobate. The structure of the head, the dentition, and the form of the paired fins, show that *Tristichopterus* has nothing whatever to do with *Dipterus*. It seems to be more closely allied to *Gyroptychius* than to any other known genus.

Tho following Gentleman was duly elected a Fellow of the Society:—

John Aitken, Esq., Darroch, Falkirk.

Monday, 19*th April* 1875.

SIR WILLIAM THOMSON, President, in the Chair.

The following Communications were read:—

1. Note of Temperature Measurements in the Great Geysir of Iceland—August, 1874. By Robert Walker, Esq.

I have thought it might be of interest to the Society to lay before it a short account of some temperature observations which I succeeded in making at the Great Geysir of Iceland, in the month of August last year. As the circumstances of my visit to the island obliged me to limit my stay at that remarkable spring to a few hours, and as, during that time and for 48 hours previously, no great eruption occurred, I fear my results must appear somewhat meagre and unsatisfactory. The very interesting nature, however, of the problem of the action of the Great Geysir, and the difficulty of securing any reliable observations at all in so inaccessible a region, will perhaps be deemed sufficient grounds for my taking up the time of the Society with these few remarks. So far as my results go, they confirm very remarkably those of Professor R. Bunsen, who, with a companion, spent more than ten days at the spot in July 1846, and to whom science is indebted for the now generally received theory of the action of the Great Geysir. An account of his observations was given in the "Annalen" for 1847, and to it I shall refer frequently in the course of these remarks.

If the difficulty of obtaining thermometer readings at various points in a column of water from 70 to 80 feet deep, and more or less in a constant state of agitation, be great, the difficulty of reaching the place at all I found to be by no means inconsiderable. Arriving in Reykjavik on the afternoon of Monday, 3d August, after a very stormy passage of nearly five days from the Clyde, we experienced, owing to the visit of His Majesty the King of Denmark, more than the usual trouble and delay in securing guides and ponies, for the long ride of nearly 80 miles up country. It is usual to take this in two stages, halting at Thingvalla, which is rather less than half way, or about 35 miles from Reykjavik. Starting with several fellow-travellers on the afternoon of Tuesday, the 4th, under orders to return to our steamer on Saturday evening, it was early morning next day when we pitched our tents on that classic plain where, on the Friday, the King was to address the assembled deputies from all Iceland. My companions decided to remain and witness this great national demonstration. After a great deal of trouble I at last succeeded in finding a native who had no scruple on patriotic motives to absent himself, and act as my guide on to Geysir; and, through the goodness of some Iceland friends, I was able to secure the companionship of a most intelligent lad of only 15 years of age, a student at Reykjavik, who, besides knowing Icelandic and Danish, could speak English remarkably well. Making an early start, then, with these two, and five horses, on the morning of Thursday, 6th August, we managed to reach the Geysir in 8 hours, meeting the king and his retinue on their way down. From one or two members of the American party, and some English travellers, who had preceded us, I learned that there were great expectations that the Geysir was at last to go off. It had erupted twice on the morning of Tuesday the 4th, but not since then, so that the king had been obliged to return, after boiling an egg at the edge of the basin. By those who were now about to follow him I was congratulated as being quite certain to see an eruption before morning; but no such good fortune was in store for us. Dr Hayes, of Arctic fame, kindly assisted in arranging the tackling of rope and cord which I had brought with me for letting down the thermometer, and one of the English party, a Cambridge man, the Rev. E. MacCarthy of King Edward's School, Birmingham, was even so good as to volunteer to remain behind and help me with my observations, an offer which I gladly accepted. I need hardly attempt to describe what travellers have so often described already, I mean the general situation and form of the Geysir tumulus, and the beautiful basin filled with pellucid water, by which this mound of deposited silica is crowned. The water in the centre seemed three or feet deep to the mouth of the funnel proper; but of course our measurements were necessarily taken from its surface in the basin, and this may account for the fact that my measurement of the depth of the funnel is 3 or 4 feet in excess of Bunsen's.

In devising the apparatus employed I was kindly assisted by my friend Professor Fuller of Aberdeen. I procured from Casella of London a self-registering maximum thermometer, which is now on the table. We had made for it a case of brass, the ends of which were made to unscrew, and were pierced with holes. The thermometer was kept in its place in this case by little wedges of cork, which, however, allowed the water to have free passage through the tube from end to end. To protect the thermometer and case against injury from the sides of the Geysir-funnel, we slid over its ends two large pieces of cork, and connected these lengthwise with slips of wood. This arrangement was found to answer admirably, unless that the large masses of cork required so heavy a weight to submerge the whole apparatus that we had some trouble in hauling it in towards the side when each reading was taken. We had also omitted to provide swivels to prevent twisting of the cord when the cap was unscrewed, and from this cause much time was lost in getting each successive reading. The following 12 were all that Mr. MacCarthy and I could obtain, though we worked well all the time we were there, unless for 6 or 7 hours when we went to rest in the boer or farm-house near by, leaving a watch, with orders that we should be called if any unmistakable signs of an approaching eruption were given. From this division of our time it resulted that of these 12 readings, one-half were taken on the evening of the 6th August, and the rest on the morning of the 7th. Allowance must be made for this, in accordance with one of Bunsen's results as to the general rise of temperature in the whole column as a great eruption is coming on.

Temperature-measurements at the Great Geysir, 6th and 7th August 1874.

Depth in feet from surface.	Observed temp. Fahrenheit.	Calculated boiling-point.	Diffs.
0	187°	210°	23°
10½	190°	224°·3	34°·3
18	197°	233°	36
27	211°	241°·8	30°·8

Depth in feet from surface.	Observed temp. Fahrenheit.	Calculated boiling-point.	Diffs.
36	243°	250°·9	7°·9
39	247°	252°·2	5°·2
45	250°·5	257°	6°·5
49·5	254°	260°·2	6°·2
54	256°·5	263°·3	6°·8
58·5	254°	266°·5	12°·5
67·5	*259°·5	272°·2	12°·7
77·5	257°	278°	11°

* Mean of two observations { 258°, 261°

Bunsen's Measurements in 1846.

6th July, 8.20 p.m.		7th July, 2.55 p.m.		7th July, 7.58 p.m.	
Height from bottom, in feet.	Temp. Fahr.	Height from bottom, in feet.	Temp. Fahr.	Height from bottom, in feet.	Temp. Fahr.
0	254·5	0	261·5	0	259·7
15·75	252·8	16·4	253·4	...	...
31·5	235·4	29·5	248·7	32·3	251·2
47·25	186·4	48·36	223·5	48·36	230·0
63	180·7	64·1	185·4	64·1	184·5

For comparison I give above Bunsen's results from the "Annalen" (1847), with the readings reduced to the same measures as my own. His heights are *from the bottom;* no doubt, because for some time after a great eruption the level of the water in the funnel is gradually rising. While I remained, the basin continued nearly full, the level of water in it not changing more than a few inches.

Bunsen draws the following conclusions from these observations:—

(1.) That, omitting small irregularities, the temperatures in the Geysir-column diminish from beneath upwards.

(2.) That the temperature at all points is dependent on the time since last eruption.

(3.) That that temperature nowhere reaches the boiling-point due to the pressure until a few minutes before a great eruption.

(4.) That the temperature about the middle height of the column comes nearest to the boiling-point corresponding to the pressure, and approaches it more nearly the nearer the moment of a great eruption.

He argues, therefore. that immediately before such an eruption only a small shock will be sufficient to vaporise a large mass of the whole column, and so to displace the whole column above. Now, it is a fact that the column is constantly subject to such shocks, which occur at intervals of a few hours, and are more frequent as a great eruption is approaching. Bunsen accounts for these shocks, which are in fact abortive attempts at an eruption, in this manner. He observes that it is a feature of most of the Icelandic warm springs that, periodically, at certain points, great bubbles of steam get formed, and rising soon condense in the colder strata above. This is well seen in these rocky cavities, 10 or 12 feet deep, which exist in that remarkable region of springs and mud-cauldrons in the immediate vicinity of the Great Geysir. I observed also something of a like phenomenon at the "quhar" by the side of the lake at Laugardalr, where we rested on the way between Thingvalla and the Geysir. At that spring, however, as the depth of water is quite inconsiderable, the effect is more of a continuous bursting of great bubbles of steam on the surface, as no condensation takes place, the water being at a temperature close on the boiling-point. Now Bunsen argues that, if at some point in the in-carrying ducts of the Geysir-column (and the existence of these ducts is proved by the constant overflow of water from the basin), the temperature of the layer of water gets raised above the boiling-point due to the pressure, owing to the great heat of the surrounding rocks, then a sudden generation of steam is the result, and a rise of that steam in the column itself. This great bubble is soon condensed, while at the same time its sudden formation cooled the water at the point in the duct where it was formed. The phenomenon, therefore, possesses a periodic character, and the explanation, it must be admitted, seems to account well for the conical water-hill, as Bunsen aptly terms it, the sudden upheaval of which in the centre of the basin is an invariable accompaniment of these subterranean explosions, often of very great violence, which are heard and felt recurring at intervals under the Geysir cone. The grand display of a great eruption, however, does not occur until the temperatures in the whole column are, by the influx of heated water, brought so near their respective boiling-points that a slight upheaval at a certain point of the tube is sufficent to carry the superincumbent layer to a point where, from its temperature and the slight diminution of pressure, a further generation of steam (instead of a condensation of that already formed) will be the result.

Bunsen has shown that the mechanical force which this action developes is fully sufficient to account for the marvellous phenomena of a great eruption. Bunsen's observations and my own agree in showing that it is somewhere about the middle of the column that the observed temperatures approach most nearly to those of the boiling-point due to the pressure. An upheaval of the layer at that point, through only a few feet, will be sufficient to generate instantaneously an additional volume of steam, the pressure of which will again further relieve that of the strata beneath, and so cause an additional volume to be generated there. The enormous force which the phenomenon of the sudden upheaval of a small column of water is thus capable of calling forth cannot be spent in a single eruptive-shot, and hence the explanation of the fact that a great eruption lasts sometimes for four minutes.

No theory of internal steam-cauldrons, filled in succession with steam and water, seems at all consistent with observed phenomena. It fails to explain how, in the abortive eruptions, no water seems to flow from the tube more than the small rivulet which steadily finds its way at the indentations over the rim of the basin. What flows over the margin, and it was great enough, considering its temperature, to cause considerable difficulty in retaining hold of the cord and rope, was due solely to the great commotion in the basin, and was apparently equal to the fall of a few inches in the level of water in the pool when the sudden upheaval had subsided. Further, Bunsen actually let his thermometer remain without injury at the bottom of the tube during a great eruption, having noted on it, a few moments before, a temperature of 9° C. below that of the boiling-point due to the pressure. The column erupted on that occasion he estimated at 43·3 metres or about 142 feet.

It will be observed, that there is a remarkably sudden rise of temperature at a particular point of the column. Thus while the rise between depths of 10½ feet and 18 feet is only 7° F., and that between 28 feet and 27 feet, 14° F., the rise between 27 feet and 36 feet is as much as 32°. I regret that I did not observe this so as to interpolate one or two additional observations between these. But Bunsen's results (*vide* 3d col.) give one intermediate measurement, his others being in remarkable accordance with mine.

We had several displays of the power of Strokkur, a smaller Geysir about a 100 yards from the Great Geysir. Unlike the latter, it can be made to erupt by throwing in turf, stones, or earth, which stop up the funnel at a point about 27 feet down, where it narrows from a width at the mouth of about 8 feet to about 8 inches. The people living at the farm-house asserted that the long interval of inaction of the Great Geysir was owing to the very frequent eruptions of Stokkur, which had been provoked by way of display during the king's visit.

Professor Tait has suggested the use of a thermo-electric junction, after Becquerel's method, to determine with perfect accuracy the temperature at any point of the column. I believe, however, that no care in packing would make it possible to transport safely a galvanometer and a thermo-electric arrangement over 80 miles of such country as one has to travel to the Great Geysir. I had the misery of seeing the package containing my thermometer repeatedly tossed from the pack-saddle, without any injury to the instrument, however, as I found by comparing it carefully on my return with one tested at Kew. The packages are fastened usually with hair ropes, and not only are these constantly getting loose, but, when a narrow part of the way is reached, the ponies, urged on behind by their drivers, charge against each other, and often leave their loads behind ere they get through.

Setting out from the Geysir at 1 P.M. on Friday (7th August), I reached Reykjavik at 6 the following evening, and, I confess, was grievously disappointed to find that our steamer was not to sail for other 48 hours, an interval which would have sufficed to complete my observations, and would, most probably, have given me an opportunity of witnessing an eruption of this world-renowned spring.

2. On the Capillary Surface of Revolution. By Sir William Thomson and Mr John Perry.

3. On the Oscillation of a System of Bodies with Rotating Portions. Part II.—Vibrations of a Stretched String of Gyrostats (Dynamics of Faraday's Magneto-optic Discovery), with Experimental Illustrations. By Sir William Thomson.

4. On the Theory of the Spinning-Top, with Experimental Illustrations. By Sir William Thomson.